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## Session 6: Discussion and Replies

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Discussion by R.M. Belkune,  
Assistant Professor in Civil  
Engineering, Indian Institute  
of Technology, Bombay, India,  
on "Offshore Caissons on Porous  
Saturated Soil" by George Gazetas  
and Emmanuel Petrakis.

At the outset let me congratulate the authors for attempting a problem of current interest. The above paper is based on use of Biot's theory for soil as a poroelastic medium. Compressibility of both solid and fluid in the pores is considered. The porous medium is considered as isotropic and the fluid flow through it is governed by Darcy's Law.

In the summary of the previous work there is no emphasis on any paper on finite element method, which I am sure would have enabled the authors to drop some of the implicit assumptions. It is not very clear if the formulation can be, although in a limited sense, extended to a general case by way of superimposing harmonic contributions, which will ultimately enable to solve for a irregular train of waves as is the case with actual sea states.

In the statement of the problem the assumption reducing the problem to plane strain seems to be too drastic for a practical case. Further, the modelling of the forcing function, also, may not adequately represent the actual sea state. The claim to "a rigorous formulation and solution" to the problem needs justification.

The foundation of caisson is represented by two cases. In the first cast, the medium is subjected to harmonic surface pressure by waves, etc. In the second case, a rigid massless strip is considered, which is undergoing rocking and swaying vibrations.

In both problems, however, there is no account of the behavior at the interface of the soil and base of caisson. Accounting for energy loss at the interface results into phase difference in the oscillations of the base of caisson and the poroelastic media. The authors have developed a generalized equation considering the dynamic equilibrium of solid and fluid parts which does not account for the flexibility of caisson base. This may be an idealized condition used for solving the equation, which may give a totally different picture of stress distribution at the base of the caisson. Influence of this flexibility in higher modes may be important. Moreover it may be further noted that the structures like caissons, jetty, etc. are not weightless. Due to the weight of the caisson there will be rearrangements of solid particles in the portion immediately below the caisson making the soil mass more dense and hence reducing porosity. The density of additional apparent mass relating the coupling between the fluid and the solid structures depends upon many factors such as properties of water, electrical charges on soil particles, temperature, geological formation of the soil mass, etc. In the definition of stress variables, the singularity associated with the toe of the caisson base needs consideration.

If the present study is to be used in a practical design the above points may be of importance as these may alter the author's findings, which are based on very much idealistic situation.

Discussion by R.M. Belkune,  
Assistant Professor in Civil  
Engineering, Indian Institute of  
Technology, Bombay, India, on  
"Seismic Design of the San Francisco  
Ocean Outfall" by O.H. Gilbert.

At the outset let me congratulate the authors for presenting an excellent treatise on a practical problem. The work reported herein is the end result of efforts put in by the experts of the various disciplines and coordinated in the best possible manner. Details regarding sliding and locking joints presented in the paper are noteworthy. The geotechnical investigation part which is most important highlighting the offshore test pit program included eight weeks of dredging and twenty-four months monitoring period: some broad features of (1) dredging rates (2) spoils disposal technique (3) short term and long term behavior of pits (4) slope stability and (5) infilling rates would have enlightened the readers by way of ready information. Excavation depths of outfall trench is an interesting aspect presented.

In the light of the statement made by the authors "potential effects of waves and currents overshadowed seismic concerns", would the authors throw some light on the following aspects of design:

(1) Is it likely that after a few years, due to waves and currents the maximum slip accounted in the design (16' - 20') in fault transition zone would occur? The outfall conduit will break even before the design earthquake occurs.

(2) Whether potential land sliding or lateral spreading of the backfill would be significant and development of excess pore pressures would cause liquefaction of the clays? How this is accounted on the stretches of outfall conduit on either side of deformation zone where there are no special joints?

(3) Would the authors project some details on the time factor estimation for repair operation; after-effects of diffusing the effluent at 1.5 miles offshore; effects during high tides and low tides; will this involve any nuisance on the beach?

(4) Although excess pore pressure development was anticipated during design earthquake even then it was assumed that clays were not susceptible to liquefaction. How far this is true for the prevailing site conditions?

Discussion by Y. Moriwaki,  
Sr. Project Engineer,  
Woodward Clyde Consultants,  
San Francisco, CA, on  
"Behavior of Clays Subjected  
to Slow Cyclic Loading and  
Large Strains" by Adel Saada  
and Louise Shook.

The authors' paper has addressed a number of issues including an important but often neglected aspect of nonlinear soil characterization—how to choose values of parameters in a nonlinear soil model from laboratory test results. The writer's discussion is related to the authors' reference to the Idriss et al. (1978) paper.

It is important to emphasize that in the referenced paper the degradation index  $\delta$  is defined based on results from strain-controlled (constant cyclic strain) cyclic tests. The linearity of  $\log (G'_n/G'_1)$  (or  $\log (E'_n/E'_1)$ ) vs.  $\log N$  relationship reported in the paper is purely empirical, and applicable only for strain-controlled test results. The linearity has been demonstrated for a number of different clayey soils since the work in the Idriss et al paper. It should be also noted that even under strain-controlled loading conditions, slight nonlinearity in  $\log (G'_n/G'_1)$  vs  $\log N$  relationship sets in for relatively large cyclic strain levels (say  $\gamma > 0.5\%$ ) after large number of cycles (say  $N > 100$ ).

Under stress-controlled (constant cyclic stress) loading conditions, the  $N$ th cycle secant modulus is generally associated with a strain level higher than that of the first cycle secant modulus. Under these conditions, the writer has also found that  $\log (G'_n/G'_1)$  vs.  $\log N$  relationships are nonlinear similar to Fig. 7 in the authors' paper. But, clearly,  $G'_n/G'_1$  from a constant stress cyclic test cannot be mixed with  $G'_n/G'_1$  from a constant strain cyclic test. A procedure to calculate  $\log (G'_n/G'_1)$  vs.  $\log N$  relationships from a series of stress-controlled cyclic tests in a manner consistent with strain-controlled cyclic tests has been presented elsewhere (Idriss, et al, 1980).

Using the procedure presented by Idriss et al (1980), a consistent  $t$ -parameter vs. cyclic strain can be obtained from a series of stress-presented by Idriss et al, (1980),  $\delta = G'/G'_1$  where  $G'_1$  are from constant strain cyclic tests and  $\delta = N^{-t}$  (see Eqs. 3 and 4 in the authors' paper).

#### REFERENCE

Idriss, I. M., Moriwaki, Y., Wright, S. G., Doyle, E. H., and Ladd, R. S. "Behavior of Normally Consolidated Clay Under Simulated Earthquake and Ocean Wave Loading Conditions." Proceedings, International Symposium on Soils Under Cyclic and Transient Loading, Swansea, January, 1980.

Discussion by O.H. Gilbert, Jr.  
 Senior Project Engineer, Woodward-  
 Clyde Consultants, San Francisco,  
 California, on "Long Term  
 Measurements of Ground Motions  
 Offshore", by Eric W. Reece,  
 David E. Ryerson, and  
 R.L. McNeil

The authors have described the design, development, and deployment of a very interesting and potentially important instrument system (SEMS) that has the capability for developing a significant data base for offshore earthquake ground motions. It is hoped that continued support of this program will lead to the development of such a data base.

However, it seems premature to suggest that there may be fundamental differences between onshore and offshore ground motion response based on two records from a single earthquake.

The authors postulate a number of factors that could modify offshore response: 1) soft and/or gassy soils; 2) wave reflections within the water column; 3) a seismic velocity profile increasing sharply with depth; and a wedge-shaped sediment profile leading to focusing or defocusing, depending on the location of the earthquake source relative to the site. However, excepting gassy soils and water column reflections, all of the above factors could be equally applicable to an onshore site.

The authors compared their observed sea/land acceleration attenuation with those predicted by two empirical (rock site) relations and found that the observed attenuation was only 13 to 23 percent of those predicted. By their nature, such empirical relations represent a "best fit" to scattered data, so deviations of a single event from the "best fit" are not compelling suggestions of a trend. The empirical relations only broadly categorize site conditions (e.g. rock site, deep alluvial site, soft site) and therefore mask the important site-related factors enumerated above. For this particular case, the writer believes that empirical relations based on deep alluvial sites may have been a more appropriate selection for the comparison.

Any rational predictive model of offshore ground motion response should consider the factors enumerated by the authors, and will require the collection of site-specific data and stratigraphy. The offshore ground motion data base that can be acquired by the SEMS system will provide valuable input for the development and refinement of the predictive models.

## AUTHOR'S REPLY

Closure by O.H. Gilbert, Jr., Y. Eisenberg,  
and D.D. Treadwell

The writers appreciate Belkune's comments concerning the seismic design of the proposed ocean outfall in San Francisco. Responses in the general areas of offshore test pit construction, wave and current effects, pore pressure development, and post-earthquake repairs are contained in the following paragraphs.

Evaluation of offshore dredging operations and the time-dependent response of the excavated pits to oceanic conditions were the primary objectives of the test pit program. Unfortunately, space limitations prevented a more complete discussion of the results of the program in the paper. However, the test pit program has been fully described by G. J. Murphy et al. (see reference list).

The writers did not intend to infer that ocean conditions overshadowed seismic concerns in all aspects of the outfall design. The maximum predicted slip of 16 to 20 feet is associated with the fault trace during the maximum design earthquake. The outfall will likely rupture during this event; the special joints are intended to limit the extent of the damage and to facilitate post-earthquake repair of the outfall conduit.

The wave defense philosophy involved the burial of the conduit and the protective riprap below the seafloor. The excavation depth was selected such that the riprap would not be exposed by littoral scour during the 75-year life of the Outfall.

It was concluded that the clays in the project area would develop some excess pore pressures during the design earthquake, but not to the point of failure. Thus, the likelihood of adverse consequences (landsliding, lateral spreading) was believed to be very low because of the extremely flat slope of the ocean floor in the project area.

The post-earthquake repair of the ruptured conduit would involve the mobilization of stored spare parts and the floating equipment necessary to excavate down to the damaged sections, remove them, and place the new sections. It is estimated that this procedure could take as little as six months to complete, depending on priorities established by local agencies. The increased likelihood of effluent contacting the shore during this period would probably not be of major concern compared to other life-line repair needs.

Bea has inquired about the selection of the design earthquake for the Outfall.

Seismic design parameters were developed by the project Seismic Advisory Board. This distinguished panel included Doctors H. B. Seed, B. A. Bolt, G. W. Housner, and the late N. M. Newmark.

The magnitude 8<sup>+</sup> event was selected based on historic seismicity - specifically, the 1906 San Francisco earthquake and on the 75-year design life exposure of the structure.

It is granted that immediately after a major earthquake, San Francisco will have more pressing problems than wastewater pollution of the ocean. However, the outfall would have to be repaired or replaced to protect the environment and public health in the long term.

It therefore seems prudent to protect the \$160 million structure from massive failure in the event of a major earthquake on the San Andreas Fault.

## AUTHOR'S REPLIES

Closure by George Gazetas and Emmanuel Petrakis

The authors agree with the moderator's appraisal (Bea, 1981) of the proposed poroelastic model for analysis of soil-structure interaction during offshore cyclic loading. They would also like to thank Dr. Belkune for his interest in their work and offer the following comments on some of his observations.

The presented method, like any engineering theory, is based on certain idealizations of the physical reality and thus it should be used, with the appropriate engineering judgement, as a tool for developing insights into the behavior of long caissons resting on cohesionless soils that may or may not contain free gas. The assumptions of the theory are by no means more sweeping than the assumptions of the classical elastodynamic theory, on which many currently used soil-foundation interaction methods of analysis are based. It is the authors' opinion (in agreement with the moderator) that the most severe of the assumptions made is that of linearity of soil response and not those mentioned by the discussor. Thus:

- 1) If linearity of the response is accepted, the effect of any irregular train of waves can be readily studied through a Fourier decomposition.
- 2) It is not clear as to what exactly the discussor means by "energy loss at the interface", but certainly the phase difference between imposed forces and resulting soil-surface deformation are accounted for in the paper (Eq. 27 and Fig. 3). This is done by enforcing the "correct" boundary conditions at the interface between the rigid foundation and the soil. Certainly, no slippage or other loss of support for the caisson has been studied; such phenomena, important as they might be, are nevertheless beyond the scope of the proposed (linear!) model.
- 3) Of course, caissons are not weightless structures; however, once the response curves of a massless plate (as those of Fig. 3) are known, one can readily account for the caisson inertia to obtain the actual structure response (appropriate formulas can be found in standard textbooks of soil dynamics). The importance of the stiffening of soil under a caisson due to (statically induced) stresses from its weight can only be studied with numerical techniques. The authors are not aware of the results of any such studies, whether modeling the soil as a one or two-phase continuum.

Closure by E.W. Reece, D.E. Ryerson and R.L. McNeil.

The discussions by Messrs. Bea and Gilbert both make two important points: 1) there is a strong need for an offshore ground-motion base, for which purpose the SEMS is uniquely well suited; and 2) one should not jump to the conclusion, based on only one data set, that offshore motions may be substantially different from onshore motions. We agree with both of these points, and concur with Mr. Gilbert's observation that deviations of a single event from "best fit" empirical relations are not compelling suggestions of a trend. Instead, such deviations should be studied to reveal the lessons they contain, and that was done by the source-modeling work presented in the paper. These data do, however, alert the profession to be on the lookout for important deviations, because they may be there and be real. We were not seeking to propound a universal rule, and we appreciate the Discussors' clarification of the point.

The SEMS is unique because: 1) it is queried acoustically, obviating the necessity of recovering a cassette or the entire instrument; 2) it can decide if an event is larger than those already recorded, and will write the larger event over smaller ones. Thus, if there are spurious disturbances, such as foreshock to a major event, SEMS will write the major event over the foreshock. Thus, SEMS would record a Gazli-type event, whereas other instruments with fixed recording times might not due to foreshock activity. This feature could be quite important if there is a high level of cultural activity, such as on a drilling or production platform. If SEMS-like devices had been in place on the platforms in the Santa Barbara Channel on August 13, 1978, some very important records might have been written. Instead, no records were written because the conventional instruments were loaded to capacity with vibrations from the platform activities.